

evidence relied on by those who see in the phenomenon of chromosome-reduction a confirmation of Weismann's theory falls to the ground. And with it, also, the hypothesis of plurilangency and continued persistence of the chromosomes suffers a serious limitation, for it is obvious that a time must soon arrive, in the sequence of generations, at which the evident chromosomes themselves can no longer consist of the telescoped chromosome units of all the previous life-cycles. In short, each chromosome that appears after the reduction in number cannot be represented in terms of the somatic units as $a+b$, but it must possess a new structure c .

Prof. Wilson very fairly reviews the evidence for and against a *qualitative* reduction (of the quantitative or numerical reduction there is, of course, no question), leaning, as has been said, somewhat in its favour, and it must be admitted that there is some indirect evidence in support of it. Perhaps it hardly falls within the scope of the author's work, but a consideration of the reversion of hybrids to the original stocks, such as indicated by Mendel's law, which has recently formed the subject of important communications by De Vries and by Correns, might have been discussed in this connection.

Exigencies of space forbid us to do more than to indicate the excellence of the treatment of the structure and development of the spermatozoon, of the phenomena of fertilisation, and of parthenogenesis. Our views as to the essential nature of fertilisation are undergoing a change in certain respects as the result of cytological investigations in this field of inquiry. We have clearly to recognise the existence of two distinct factors in the process. The one is concerned with the stimulation of the egg, which is thereby impelled to segment and to develop into a new organism, the other is involved in the fusion of the two sexual nuclei.

Boveri's experiments long ago showed that a fusion of the male and female nuclei was not essential to the segmentation and organised development of an egg. He succeeded in fertilising non-nucleated fragments of echinoderm eggs with the sperms of another species, with the result that larvæ exhibiting the paternal characters only were formed. These experiments were for some time regarded as not being free from objection, but they have been repeated with similar results. Again, as Loeb has recently shown, it is possible, by treating the unfertilised eggs of *Arbacia* with a solution of magnesium chloride, to cause them when replaced in sea-water to give rise to normal larvæ. And once more, Nathansohn has proved that, in the case of *Marsilea*, a sufficiently high temperature suffices to excite parthenogenetic development in the oospheres of these plants. Even in many normally fertilised eggs it has been repeatedly shown that the stimulus which starts the karyokinetic processes in the egg comes from the cytoplasmic (centrosome) portions of the sperm rather than from its nucleus.

As regards the significance of the nuclear fusion, although we are as yet unable to speak with certainty as to its proximate or efficient cause, there can be little doubt but that its teleological significance is to be sought in the fact that these bodies contain in themselves the physical basis of heredity, and thus by their coalescence the hereditary qualities of both parents are mingled in the offspring.

J. B. FARMER.

METHOD IN PHILOSOPHY.

Die Transzendente und die Psychologische Methode.

Dr. Max F. Scheler. Pp. 181. (Leipzig: Dürr'schen Buchhandlung, 1900.) Mk. 4.

IN opposition to the positivism which avers that if we take care of facts method may be left to take care of itself, Dr. Scheler claims that the history of thought, its continuity notwithstanding, shows abundantly that each fresh conquest in knowledge is preceded by a definite, if often half-conscious, breach with outworn method. Kant's historic mission has been fulfilled, and, after a century's probation, the time has arrived to pass beyond him. Not, however, by the adoption in philosophy of that psychological method which, discredited in Condillac and Hume, has been encouraged by recent advances in technical psychology to essay rehabilitation in more plausible forms. Dr. Scheler is with contemporary psychology in its reaction in favour of real as against formal principles, development as against finality, the historical as against the mathematical temper. He is with the Kantian in his recognition of the *quaestio juris* and in his advocacy of an inverse or "reductive" method. In the result he accepts a formula from Eucken—that of a regress from "the well-founded phenomenon" of a culture embodied in a coherent aggregate of institutions to the real forces of which it is the living and still growing product. *Arbeitswelt* and *Geistige Lebensform* are the catchwords of this "noölogical" method.

Dr. Scheler's discussion of "transcendental" method, *i.e.*, the inference from an accepted group of facts to the principle which can and can alone explain them—what evidence of the "alone" could be adduced it is hard to see—directs its main attack, not against its inverse character, but against (1) its static nature due to acceptance of an immutable starting-point, (2) its formalism in its conclusion to grounds of a merely logical kind, (3) its intellectualism with its consequent neglect of "three-fourths of life." Its alleged "synthetic propositions *a priori*," *i.e.*, propositions at once instructive and necessary, are really experiential. The starting-point is really dependent on psychology. Change the period and get a different psychological "climate," and you will find that the transcendental presuppositions will be different. But if so, the psychological ground-propositions will be complete in themselves, and formal conditions established by transcendental deduction are superfluous. The only regress which is not simply a doubling of the data must be towards real, that is, actual and active principles. And the data are neither unchanging nor purely rational.

This general appreciation Dr. Scheler reinforces by a detailed treatment of space, time, causation, and personality. As regards space, the temptation to strengthen the charge of formalism by putting Kant out of touch with a perceptual world has proved too strong for Dr. Scheler. Kant's "empty" space probably means only that all particular contents of space can severally be thought away without altering our space-apprehension. Kant's space is voidable rather than void. Geometer's space, while it is in one sense an abstraction, is not only not a generic concept, but not a concept at all, if the argument as to whole and parts in Kant's metaphysical exposition is to stand. The psychogenetic problem of the perception of a third dimension is irrelevant to Kant's nativism.

The bearing of "metageometry" upon Kant's doctrine of space is still *sub judice*. It is open to the Kantian to maintain either that "bent" spaces fall within Euclidean space, multidimensional spaces being an hallucination due to abuse of algebraic symbolism, or, with Mr. Bertrand Russell, that if Euclidean space is experiential, yet some "form of externality" is *a priori*. Dr. Scheler is, however, throughout the discussion suggestive, if inconclusive.

Less satisfactory is his treatment of causation. The statement that the conception of uniformity is foreign to the Greek period is absurd. That the period of Roman decay was one of lawless happenings is not true in the sense of p. 73, and Dr. Scheler does not save himself by after qualification, in view of the exaggerated position of p. 69, that the causal category which makes natural science possible as a science of experience would make historical science as a science of experience impossible. The treatment of time and the self is relatively slight.

The inadequacy of the psychological method is to be found in its equivocal use of the term "facts of consciousness." Either it is Protagorean and anarchical, and the objects of all sciences and nesciences are on a dead level of "psychical existence," or there are realities which transcend this accommodating rubric. Idealism is prone to the epistemological fallacy, as positivism is prone to the phenomenalist fallacy.

If, however, neither transcendentalism with its reduction, nor psychology with its grip on something real—even falsities—can satisfy us, we must, in default of other probable courses, cast about for some syncretist formula uniting the truths and discarding the defects of both. Dr. Scheler declines Sigwart's irenicon, because of the primacy it involves of the moral and volitional element in life. Surely this is not ineradicable from Sigwart's formula? Rejecting this, and the solutions of which it is the type, he falls back upon the endeavour of his teacher, Eucken, to make jettison of all in both methods that offends the time-spirit, and to fashion what is left, with the aid of something which both had left out, into a non-absolutist, non-sceptical scheme, hereafter to be more fully developed.

H. W. B.

OUR BOOK SHELF.

First Stage Botany, as Illustrated by Flowering Plants.

By Alfred J. Ewart, D.Sc., Ph.D., F.L.S. Pp. viii + 252. (London: W. B. Clive and Co., no date).

THE author sets forth in the preface that his primary object in writing this book was that of satisfying the requirements of students preparing for the elementary stage of the Science and Art Examinations. A glance through its pages suffices to prove that this end is everywhere kept to the fore. Even the figures, which are very numerous, are labelled all over in large type so as to enable the student, with the minimum expenditure of time and trouble, to get up the maximum amount of facts. In the text the treatment is on analogous lines, and probably the student possessed of a good memory might, with this book as his mentor, succeed in passing a fair examination. Beyond this we have failed to discover why the book was written; and when its author goes on to state that it is also intended to serve as an "efficient introduction to Botany," we simply cannot agree with him. The character of the book is too dogmatic, and too little is left to the student. Indeed, a sentence contained in

the preface, advising the student to obtain specimens and "verify upon them the statements made in the text," gives the key to the entire book. Not merely verification, but the fostering of a spirit of *inquiry* ought to be the chief aim of a teacher, and it is this aspect of the matter which we miss in the volume before us. In the paragraph on geotropism (p. 211) this phenomenon is defined as the "tendency of the radicle or main root to grow towards the centre of the earth"; a very inadequate definition both from the point of view of fact and theory, and one of little or no scientific value to the student.

Some subjects, e.g. obdiplostemony, are introduced which would have been better omitted. Unless fully discussed they are of no value educationally, and the space they occupy would be better taken up by a more extended treatment of the more elementary matters. Although, in a general way, the book much resembles others of its class, save, perhaps, in the compression of an unusually large number of facts into its pages, it is but right to add that actual errors are remarkably scarce.

The Principles of Magnetism and Electricity. An Elementary Text-book. By P. L. Gray, B.Sc. Pp. xvi + 235. (London: Methuen and Co., 1901.) Price 3s. 6d.

THE number of elementary text-books on magnetism and electricity probably exceeds that of text-books on any other subject. One would, therefore, naturally expect that anybody attempting to add to their number would do so with a due sense of responsibility, and endeavour to produce a book which might be regarded as surpassing those already in existence either in accuracy of exposition or in freshness of treatment. A careful perusal of the book before us has forced us to the conclusion that the author is destitute of all sense of responsibility, and not afraid to scatter error broadcast with a light heart. Seldom has it been our lot to come across an elementary text-book so full of glaring errors so boldly stated. On p. 18 the author describes a vibrational method of comparing the moments of two magnets in which the moments of inertia of the magnets are not even referred to! On p. 15 we have the startling assertion that in the case of diamagnetic bodies "the induced magnetisation is *at right angles to the field*" (the italics are the author's!). Could there be a greater confusion of ideas than that exhibited by the following sentence? (p. 151): "A pole of strength m will have $4\pi m$ lines of force proceeding from it, so that, if a transverse narrow cut be made across a magnet which has σ lines per sq. cm. in any normal cross-section, the field in the narrow slit H will be equal to $4\pi\sigma$." The author measures magnetic force in *dynes*, and difference of potential in *ergs*. On p. 162, in connection with the induction coil, we read: "Trowbridge has recently obtained sparks nearly seven feet in length, obtaining an E.M.F. of 3,000,000 volts, *the primary current being supplied from a battery of 10,000 storage cells*" (the italics are ours). Is the author serious, or does he intend playing a practical joke on his reader, by suggesting that any sane person would use 10,000 storage cells for supplying the primary of an induction coil? Had he taken the trouble to refer to Prof. Trowbridge's papers, the author would have found that the arrangement used for obtaining the 3×10^6 volts had nothing whatever to do with an induction coil. On p. 163 we have the sentence: "The total value of the magnetic force within a circuit is known as the magnetic *flux* through the circuit." Now, what does the author mean by "the total value of the magnetic force within a circuit"? When touching on technical matters, the author does not scruple to make various erroneous statements with an airy assumption of superior knowledge. "Theoretically," we are told on p. 166, "every dynamo could be used as a motor and every motor as a dynamo. In practice, however, this power of reversibility is not used." Again, on p. 170, we read: "Owing to the self-